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Air Blasts and Quakes

on the

KOLAR GOLD FIELD.

BY

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AIR BLASTS AND QUAKES.

By W. F. SMEETH.

In my Report for the year 1899* I drew attention to the fact that "air-blasts," by which term is usually meant a splitting off of small portions of the reef quartz with a considerable amount of violence and noise, were frequent in the quartz of the Champion Lode, Kolar Gold Field, and often resulted in serious injuries to the workmen underground. Since then I have collected some information about these remarkable phenomena which I purpose to relate here; unfortunately my opportunities of experiencing these phenomena or of personally visiting the places where they have occurred have been few and the information available in many of the cases quoted is much less complete and precise than I would like it to be. The present notes must be regarded merely as a progress statement calling attention to certain salient features and suggestions with a view to securing more careful observation and eliciting further facts, confirmatory or otherwise, in future cases.

Previous to the year 1900 I had received many reports of air-blasts which did not appear to me to call for particular attention except in regard to their frequency. They did not seem to differ materially from the phenomena which are known to occur in Cornwall and which have been described by Prof. Le Neve Foster who imitated them by screwing up some sheet glass between the plates of a copying press thereby causing portions of the edges of the sheet to splinter and fly off On the Kolar Gold

* Report of the Chief Inspector of Mines in Mysore.

Field the air-blasts which I speak of used to occur in the quartz of the Champion Lode and consisted of the flying off of fragments of quartz from the working faces with explosive violence. In September 1900 an "air-blast" on a very much grander scale occurred in air-shaft, Ooregum Mine, at a vertical depth of about 650 ft., which was distinctly heard and felt at surface for a distance of between one and two miles from the shaft and which caused a considerable amount of damage to the shaft at the place of occurrence. This was the beginning of a series of these larger phenomena which have continued ever since and which I shall call " quakes " Though commonly included in the term "air-blast," they appear to me to be practically earthquakes on a small scale and I shall deal with them separately from the ordinary air-blasts as they differ so much from the latter in actual magnitude and probably also in essential predisposing conditions.

Air-Blasts.

These smaller phenomena occur most commonly in the quartz reefs, in which I have observed them myself on more than one occasion. I have also received information of their occurrence in the dolerite dykes, which traverse the schists and quartz reefs of the Field, and also in the schist itself in places far removed from the reefs.

The best examples which I have seen were in the Ooregum Mine in the stope below the 1160 feet level south of Wallroth's shaft on the No. 2 East Reef. (See point x, Plate II, Fig. 1). The reef was $2\frac{1}{2}$ to 3 feet wide lying tightly between well defined walls of hornblende schist and all along the bottom and ends of the stope the quartz kept up a fairly continuous fusillade of small pieces which flew off the faces with considerable violence. A faint crumbling or

Case 1.

crackling noise was audible most of the time and every now and then there was a report something like that produced by a squib or cracker and sometimes a sharper report approaching that produced by a small detonator. The quartz on a freshly broken face was of a translucent bluish grey colour which rapidly changed to white owing to production of numerous minute cracks, just as though the whole reef was beginning to give way under pressure. The production of these minute cracks caused the continuous crackling sound and every now and then a small grain would vanish with a little puff, apparently going to powder, or a small piece up to 2 or 3 ozs. in weight would fly out with a report. I watched this for some time and came to the conclusion that the effects noted must be due to the weight of hanging wall (the dip being about 55°) on the margins of the quartz. A considerable amount of ground had been stoped above the point referred to as may be seen from the diagram, Fig. 1. Consideration of a number of other cases however suggests that the superincumbent weight is not the sole cause of the effects noted, otherwise we should expect to get similar effects in many other places where the crushing weight is as great or greater; such a connection is however not borne out by experience as air-blasts occur frequently where there is no such obvious pressure and fail to occur where such pressure undoubtedly exists. Some further explanation must therefore be sought and in seeking for this I cannot do better than begin by quoting the remarks which Mr. P. Bosworth Smith, Superintendent of the Tank Mine, has kindly sent me in connection with an air-blast in the schist which he himself observed. These remarks are particularly valuable as they are made by a trained scientific observer, a condition often lacking in the reports obtainable during the

ordinary course of mining work. Mr. Bosworth Smith says:—

Case 2.

"Regarding our conversation on "quakes" and "air-blasts," I have been fortunate enough to witness several of the latter and have seen such occurrence in "quartz," in "trap" and in the "country rock." These air-blasts in quartz and in trap are, I believe, common on the Field, but a blast in the schist away from the lode is uncommon and I have only seen one, nor have I heard of any others; I enclose a small tracing giving the locality of the occurrence. About 200 feet south of Walker's shaft in the 530 feet level we started a cross-cut east to open up our east lode. Visiting the end when the drive was about 60 feet in, I had just reached the machine bar when a sharp explosion took place on the face of the rock in the end. The rock drill which had been working a minute before was stopped as I came up and the coolies working in it had been called away from the drill in order that I could proceed straight to the working face. There was consequently nothing to interfere with my seeing and hearing the blast. The blast was as if an ordinary detonator had been fixed on the face of the drive covered with a small heap of sand and fired. The explosion sounded exactly similar and there was a small spit of fine dust thrown away from the end. There was no previous crackling of the rock giving warning such as often occurs in the quartz and the trap blasts, nor were there any subsequent smaller explosions.

The crack of the explosion was quite unexpected and very startling. The force of the explosion would probably have been sufficient to seriously injure the face and eyes of any person standing and examining closely the place where the explosion occured.

"These "air-blasts" have interested me for some time and several years ago I wrote to the Kolar Gold Field Mining Board giving my opinion that the cause of the phenomenon was similar to that of the explosion of a Prince-Rupert's drop or the bursting and crackling of a toughened glass globe. The glass in both these cases being very suddenly cooled, enormous internal strains are set up; these molecular strains are in equilibrium when the glass is whole and may be likened to a complicated system of threads all pulling violently on one point, but all neutralising one another; cut one of the threads and the whole system will suddenly collapse. In the case of a Rupert's drop the nipping of the thread of glass forming the tail upsets the balance of the internal strains and the whole mass flies apart with a loud report. Break a toughened glass globe and the pieces into which it is resolved will subdivide of themselves and will in many cases go on breaking themselves up for days afterwards, showing that in a solid these internal strains when not too violently thrown out of equilibrium take some time to assert themselves.

"The air-blasts that occur in the quartz may be caused by the pressure of the walls of the lode.

The lodes show many evidences of great pressure and this has in places caused movement of the walls and the consequent *slikensides.* Where the movement has not taken place and the lode is tight the quartz will crackle and air-blasts may be expected. I have not seen and have never heard of the occurrence of air-blasts in the loose portions of the lode.

"The air-blasts that occur in the "trap rock" are, I think, undoubtedly due to the strains set up by the sudden cooling of the original melted material, so that where the matter is coarsely crystalline there are few if any blasts, but in the fine grained trap on the fringes of the dyke the rock is in an extremely strained condition and flies very rapidly. At Tank Mine we had a very excellent example of this as we cross-cutted east at the 850 and ran for some distance in the edge of the dyke; here sharp splinters from the roof were continually being shot off, and in consequence many of the coolies received rather bad cuts. For a long time after the drive had been cut it was possible by taking a hammer and striking along the roof to set the whole place crackling and splintering again. When this crackling is going on vigorously small luminous flashes can be seen in the dark similar to those produced by the dead blow of a hammer on the rock or those produced by falling of rock in a pass underground.

"It is more difficult to make out what would create a small area of highly strained rock in the case

of our 530 cross-cut explosion. The end was very tight and was in fairly homogeneous hornblende schist breaking off to large head joints which dipped west with the ordinary banding of the rock. This carried a few small strings of calcite varying in width from 1/32nd to ½ inch and running parallel to the schistosity of the rock. These may have been introduced either by the whole rock being split up parallel to its schist lines and the calcite being carried into the fissures produced, or the little seams of calcite may have been crystallised out when the general metamorphism of the hornblende schist took place. Either cause might produce local strains: the former would be a small example of the fissure or lode pressure and seems unlikely, as if open fissures had been produced liable to be subsequently filled with new matter, the fissuring pressure would have been lost when the cracks formed. It also seems unlikely that the crystallisation of such strings of calcite could have caused sufficient strain to have made this blast. It seems more probable that during some of the great metamorphic changes which the hornblende schists have undergone, some chemical change took place by which the calcite strings were formed and that these changes introduced the local strain. My reason for supposing that the occurrence was in some way connected with the seams of calcite is that in many other places in Tank Mine where levels have been driven through rock seamed with calcite bands the rock is always "airing," as the miner calls it, *i.e.*,

after being left some time, although quite solid to start with and having no weight to carry, the streaky rock is perpetually flaking off. Though these flakes come away slowly and without anyof the ordinary features of an air-blast, I am convinced that the splitting is due to the rock having been in a state of internal strain.

"Whether the schists of the Kolar Gold Field exist as a sharp synclinal fold or not, it is undoubtedly true that they have been subjected to an enormous east and west pressure and that where this has not been relieved either by proximity to the surface or to some loose fissure underground the schists and the included dyke and vein rocks exist in a state of great strain and the removal of the enveloping pressure allows these *outward pushing strains* (if they may be so called) to exert themselves more or less rapidly and to disintegrate the rock.

"It is difficult to prove, but I think that the "weathering" of the tailings found to be necessary on this field to allow the cyanide to get at the enclosed particles of gold is due to the formation of cracks in the quartz and that these have resulted from the relief of the enclosing pressure."

The foregoing notes contain in the first place a very clear account of a small air-blast in the hornblende schist and in addition several suggestions as to the nature and associations of these phenomena generally.

With regard to the former, there are two very important points to be noted, the first of which is that the

blast took place in the end of a cross-cut, far removed from either the east or west Lode and in a situation in which there can have been no great pressure due to the overlying rock being left unsupported by reason of surrounding excavations, and the second is that the blast did not cause a lump of the rock to be thrown out, but only some dust or sand indicating that a small piece of rock in the face had been shivered to fragments. With regard to the first point, *viz.*. that the pressure due to depth was neither sufficient nor of a nature which can be regarded as likely to produce the effect described, the same contention may, I think, be admitted in the case of several other blasts which have been reported to me. For example :—

Case 3.

While Oakley's shaft (vertical), Ooregum, was being sunk, a great deal of trouble was encountered about the level of the 1060 ft. cross-cut from Taylor's shaft, both in the shaft and in the cross-cut. Small air-blasts were of frequent occurrence in this locality accompanied by sharp reports and by the fall or projection of fairly large pieces of "black rock"—as the hornblende schist is usually called by miners. A face or roof which on one day was perfectly sound would, the next day, be found much fractured and scaled. Now the locality of these occurrences was about 450 feet west of the lode in solid back rock of fine compact texture and the only excavations were the shaft itself and the cross-cut entering it at right angles. It does not seem that in such a case the superincumbent pressure could of itself produce the effects noted and it is very significant that these blasts did not occur to any noticeable degree either higher up or at lower points to which the shaft has subsequently been sunk.

Again, Capt. Johns, Chief Underground Agent of Champion Reef Mine, informed me that he heard one day a distinct air-blast in the ore-shoot on the west side of Tennant's shaft, about 1550 feet from surface. The blast was marked by a sharp report and the fall of a number of small pieces of rock down the shoot. The whole of Tennant's shaft (vertical) is in black rock and the ore-shoot is an excavation in black rock alongside the shaft, and besides these there were no excavations anywhere near, the reef being over 100 feet east of the ore-shoot and being practically intact (*i.e.*, without stopes) for 200 or more feet above this level. In this case not only is the superincumbent pressure an insufficient cause for the blast, but it would appear that a blast may occur in a place which had been excavated a considerable time previously and left undisturbed save for the tipping of the ore into the shoot. The shoot, so Capt. Johns informed me, was empty at the time, which I think disposes of the chance that the blast which he heard was due to the splitting up of a piece of quartz recently dumped into it.

Case 4.

On several occasions air-blasts displacing large pieces of rock have been reported to me from Edgar's shaft on the Mysore Mine. This is a large circular shaft, at present about 700 feet deep, which is being sunk a long way west of any other workings, in a remarkably compact and massive hornblende schist so massive indeed that the term schist is a misnomer. In a couple of these cases which I carefully investigated, there seemed to be no alternative but to accept the statements of those present that a loud report had occurred followed by the falling of one or two fairly large pieces of rock. From the nature of the sides of the

Case 5.

shaft there seemed to be no room for the breaking off of any loose shales of rock such as may occur in obliquely bedded and shaly ground and I am inclined to believe that the cases I refer to were due to the genuine splitting off of fragments from the solid rock.

Case 6.

There is one more case to which I must refer and which takes us back from black rock to quartz.

In driving the 1940 ft. level, Champion Reef, air-blasts have recently been frequent causing the roof of the level to split and fly off in small pieces. Many such blasts occurred south of Ribblesdale's shaft and were a great nuisance, but north of Carmichael's shaft they became so bad that driving had to be suspended until the blasts played themselves out. The quartz of the reef at this point is 10 ft. wide and shortly after the level was opened for some distance the quartz in the roof commenced to fly off and gradually worked itself out in the form of a wedge tapering from the roof of the level upwards for 8 or 9 ft. The effects then ceased or became so diminished that it was possible to resume work and timber up the roof. In this case the 1940 ft. level is the bottom level but one, the ground being intact below save for the 2040 ft. level and intact above up to the 1840 ft. level. In fact there is practically no stoping at all for a long way above the point referred to, the ground being intact right up to the 540 ft. level except for Carmichael's shaft itself and the levels running from the shaft. There is therefore practically no pressure of importance on the quartz forming the roof of this level such as would be the case if much of the reef had been stoped out in the neighbourhood. Neither can the pressure due to mere depth be sufficient to

account for the phenomena in such hard and rigid materials as hornblende schist and quartz and this is supported by the fact that the phenomena have not occurred uniformly along the level, nor have they occurred in the 1840 ft. level above nor in the 2040 ft. level below.

From the foregoing six cases I think it may safely be admitted that superincumbent pressure is not the cause of these air-blasts and it is quite an open question whether such pressure, when present, increases or diminishes the tendency for such blasts to occur or the intensity of those which do occur.

In *Case 1* it is true that the quartz was under a certain amount of pressure on account of the stopes immediately above it and that the small air-blasts and cracklings were almost continuous. But if such pressure is not necessary for the production of the blasts and is not the cause of them, it is at least open to us to surmise that here, in the case of quartz suitably constituted for the development of air-blasts, the superincumbent pressure may very possibly have had the effect of increasing the number of blasts and diminishing their intensity, so that, had the same faces been exposed without being subject to pressure, the tendency which gives rise to the phenomena would have relieved itself in fewer blasts of greater magnitude. In all the other cases which I have cited—with the exception *Case 2*—and in which superincumbent pressure may be neglected the blasts, so far as my information goes, have been more intense *i.e.*—greater noise and more rock displaced—than in the numerous small effects described in *Case 1*.

Leaving aside then the pressure due to the weight of overlying rock as inadequate to produce the effects

described, we must seek for some other causes for these phenomena. I may say at once that I am quite sure that "air-blasts" have no connection with any store of air or gas in the rock and that nothing which can be properly regarded as a blast or explosion of any kind occurs. The word "air-blast" is a miner's term for a phenomena which resembles the effect of firing a shot in so far as it is accompanied by noise and by the displacement of rock, but there, in my opinion, the resemblance ceases.

It is quite another matter to go beyond this and explain what an air-blast is and how it is caused and at present I have no simple or satisfactory explanation to offer, but I shall briefly allude to a few suggestions.

Mr. Bosworth Smith, in the notes which I have quoted above, suggests an analogy between the air-blast and a Prince Rupert's drop and I do not think that any one who has observed the two phenomena could fail to be struck with their similarity. In the one case we have a drop of glass which owing to sudden cooling is in a condition of intense internal strain and in which these strains result in tensions tending to contract the outer skin or sheath of the drop and which strains maintain their equilibrium only so long as the outer skin remains intact. As soon as the outer skin is injured, or fractured, equilibrium is destroyed and the unbalanced strains cause a disruption of the whole mass resulting in the reduction of the drop to powder usually accompanied by an audible crack or report. In the case of the air-blast we have a portion of rock—whether quartz, black rock, or dolerite dyke—which is in a condition of strained equilibrium and which on disturbance of this condition of equilibrium flies into larger or smaller fragments or even into powder.

The case cited by Mr. Bosworth Smith (Case 2), in which a portion of black rock flew to powder, and the disappearance of small pieces of quartz into powder, which I myself observed (Case 1), point to a very close analogy between the air-blast and the Prince Rupert's drop. It is true that in the majority of cases what is observed is the report and the fall or throwing out of pieces of rock, but this only means that the internal strains are less minutely distributed than in the two former cases or that they are sufficiently relieved at the boundaries of the projected masses to permit of the latter remaining intact for the time being. Further, it may be remarked that the case cited by Mr. Bosworth Smith is exceptional in that the only thing that occurred was the disruption of a small piece of black rock and that if several good sized pieces of the rock had been projected outwards at the same time it is more than doubtful if he would have observed the fact that a small piece had gone to powder.

In the cases which I myself noticed in quartz (Case I) the observations were by no means easy or pleasant. Standing with my back to the footwall I could see the quartz which formed the bottom of the stope cracking and becoming white and opaque, while every now and then a small piece would fly off with a loud crack. After some time I ventured close to the quartz face, shading my eyes with my fingers, and observed a series of small curved cracks come into existence with a crinkling noise. These cracks were disposed in a fringe round a translucent piece of quartz about $1\frac{1}{4}$ ins. long by 1 inch wide. While these were forming a small piece about one eighth of an inch in diameter on the left side twinkled out leaving a small cavity and a few moments afterwards a similar piece on

the right top disappeared. After this I got up and watched the spot while listening to the small blasts going on around. About five to ten minutes afterwards the central piece flew out towards my left front with a crack like a gun cap From these observations I conclude that the quartz was in a condition of strained equilibrium, that the equilibrium was slowly being disturbed and re-established disruptively and that the disruptive effects were marked by the formation of cracks, the flying to powder of small fragments and the projection from the face of larger fragments. In these phenomena we have illustrations of all the effects which are described in connection with these air-blasts with due allowance for variations in magnitude.

It seems to me that the phenomenon of small pieces flying to powder disruptively as seen by Mr. Bosworth Smith and by myself makes the analogy with the Prince Rupert's drop a very strong one and it may be as well to point out that in all probability this phenomenon is a constant concomitant of air-blasts, even though it has been specially observed only in two instances. In Mr Bosworth Smith's case it was the sole effect and in my case it was noted only because the strained area was very closely watched. As a rule cases such as those observed by Mr. Bosworth Smith would pass entirely unnoticed and few people would feel called upon to examine an incipient air-blast as closely as I did in the instance cited. While therefore the minute disruption into dust is probably common in air-blast localities, the fact that it has not been more frequently noticed or recorded is easily explained.

If, then, it is admitted that the quartz or black rock or dyke is in a condition of severe strain, somewhat

analogous to that of a Prince Rupert's drop, at those places where air-blasts occur, the next point for consideration is the way in which such strain may be produced.

On this point I have not been able to arrive at any satisfactory conclusions and must content myself with discussing some tentative suggestions and I shall begin by referring to Mr. Bosworth Smith's views as expressed in the note quoted above.

Mr. Bosworth Smith discusses the cause of the strains in the three very different classes of material in which air-blasts have been observed *viz.*, in the quartz, in the trap dykes and in the hornblende schist and his views may be briefly stated as follows:—

In the quartz the strains may be caused by pressure of the walls of the lode. This pressure is not that due to the weight of overlying rock, as I have already shown that the prevalence of air-blasts bears no relation to the variation in superincumbent pressure due to the excavation of stopes, but it is the regional pressure to which the area has at times been subjected and which has resulted in the crushing and folding of the schists and of the quartz reefs. Such pressures would doubtless cause great internal strains in the rocks which would in places be relieved by crushing or movement and would in other places persist as irregularly distributed strains owing to the rigidity of the materials involved.

In the trap dykes it is suggested that the air-blasts are due to strains set up by the sudden cooling of the originally molten material of the dykes and that the effects are more noticeable on the edges of a dyke, where it is fine grained owing to more rapid cooling, than towards the middle where the texture is coarser.

In the hornblende schist, the presence of small strings of calcite is noted, but the suggestion that this calcite may be the cause of the pressure which gives rise to air-blasts, a favourite hypothesis amongst the mining men on the Field, is rejected in favour of the view that the pressure is in some way due to the metamorphic changes which the schists have undergone, the formation of calcite being a concomitant effect of chemical alteration.

Finally Mr. Bosworth Smith sums up in the following words—

"Whether the schists of the Kolar Gold Field exist as a sharp synclinal fold or not it is undoubtedly true that they have been subjected to an enormous east and west pressure and that where this has not been relieved either by proximity to the surface or to some loose fissure underground the schists and the included dyke and vein rocks exist in a state of great strain and the removal of the enveloping pressure allows these *outward pushing strains* (if they may be so called) to exert themselves more are less rapidly and to disintegrate the rock."

I will now examine the views above expressed somewhat more in detail and I may begin by stating that the explanation that these air-blasts are essentially due to internal strains resulting from the enormous pressures to which these rocks have so obviously been subjected had for a long time appealed to me as being in the main correct and up to comparatively recently I should have agreed with Mr. Bosworth Smith on this point. For some time past however I have been in considerable

doubt as to whether the explanation is a satisfactory one and without attempting to arrive at any final solution I shall endeavour to present some other suggestions for consideration and to criticise the views expressed above.

Firstly, then, as to air-blasts in the Trap Dykes.

Trap dykes.

Mr. Bosworth Smith's suggestion that these are due to strains set up by the cooling of the molten dykes appears to me to be very reasonable and very sound. We know that the trap has cooled and contracted and the chances of strains being produced during contraction are great. The fissuring or jointing of lavas and dykes is, I think, largely due to internal strains due to contraction which have exceeded the limits of strength of the rock and have caused rupture. The dykes on the Kolar Field possess well marked systems of joint planes and the blocks formed by these planes exhibit spheroidal weathering in a very marked degree. This jointing and spheroidal weathering is noticeable for a considerable distance below surface, but is of course most marked on surface and where surface waters have circulated. It may be contended that these features are entirely due to weathering, but personally I am inclined to believe that they are essentially due (particularly the well marked spheroidal shells) to internal strains which tend to cause the joint blocks to assume the spheroidal structure, the actual development of such structure being either only potential or remaining invisible until the blocks come within the sphere of weathering agencies.

If this be correct, it is not difficult to understand why portions of a dyke when broken into underground should sometimes fly off or scale. The breaking down of a dyke by explosives would doubtless effect the relief of

many of the strains near the exposed surfaces and would cause re-arrangement of others which in favourable cases would cause pieces of the rock to crackle and fly. I have seen pieces of concentric shells scaling off the face of a perfectly fresh dyke at a depth of about 1,000 feet from surface, though there was no indication of any general concentric structure in the rock.

If, then, the strains which cause air-blasts in the dykes are due to contraction on cooling the molecular condition of the dyke material must be very different to what it would be if the whole mass were in a condition of regional compression as suggested in the last quoted para of Mr. Bosworth Smith's note. If the one explanation is sufficient to account for the phenomena, the other is unnecessary. We might suppose the dyke to have been internally strained during cooling and afterwards to have been subjected to great compression along with the surrounding rocks, in which case the latter pressure would be removed at the point where an opening was made into a dyke below ground and there the original strains would re-assert themselves and produce the blasts unless they had been obliterated by molecular re-arrangement. Even in such a case the phenomena would still be essentially due to the contractual strains modified, perhaps, by the compressive strains within the dyke material. Apart from such an assumption it is more important to note that the dykes on the field exhibit none of the usual signs of having been subjected to great pressure and which are so obvious in the surrounding schists and quartz reefs; they are not at all schistose and under the microscope the constituent minerals show no signs of crushing or deformation. The dykes are much younger than the schists and quartz reefs and have been injected into two sets of cracks or fissures,

one set running north and south and the other east and west, and, though the molten material may have been under considerable pressure at the time of injection, there is nothing to show that the dykes have been subjected to any pressure after solidification ; on the contrary it is more probable that owing to cooling and contraction the pressures within the dyke mass are negative and tend to cause the formation of joints and cracks.

With regard, therefore, to air-blasts in the dolerite dykes it will be seen that I agree with Mr. Bosworth Smith in supposing them to be due to the internal strains set up by contraction on cooling from a molten state, but that I do not agree that there is any evidence to show that the dykes have been subjected to pressure since soldification or that the air-blasts are due to strains due to compression.

Hornblende Schist.

I next pass on to the question of the strains in the hornblende schists which constitute the country rock of the Kolar Gold Field. These schists are for the most part fine grained holocrystalline rocks consisting of hornblende, felspar and sometimes quartz, usually with a schistose structure. They are the metamorphic representatives of a very ancient series of diabasic and basaltic lava flows which have been folded into a sharp syncline by great east and west pressures and in which the original augites have long since completely changed into hornblende. No one can for a moment doubt that these rocks have been squeezed under enormous pressures at some period of their history and, if it is found necessary to assume that they are now in a condition of great internal strain, it is not unnatural to jump to the conclusion that such strain is a residual

effect of the compression which is known to have taken place. This is the view taken by Mr. Bosworth Smith who considers that these compressional strains still persist locally, having been relieved in places by movement or crushing of the rock material or in other places by the formation of loose fissures. It is also the view which I myself favoured until recently, but of which I am now inclined to doubt the correctness.

We must admit that at one time the rocks were under great regional pressure sufficient to bend them into sharp folds and to cause slow viscous movements in places. At the time when such pressure was active there can be no doubt that the present rocks were buried under a great thickness of superincumbent schists which have since been removed by denudation and it is quite certain that this active pressure has long ago ceased to exist. The question remains as to whether the intense condition of strain which must have existed throughout the mass of the rocks at the time when the pressure ceased to be active still persists or whether it has been partially or wholly dissipated. I am inclined to believe that not only has it been wholly dissipated, but actually reversed so that the rock instead of being in a state of compression, is in, what may be loosely described as, a state of internal tension.

Let us consider the history of the rock for a moment. First of all we have a great thickness of basic lava flows which by earth movements became crumpled up into great folds and more or less sheared and deformed. The earth movements, which were probably very slow and prolonged, gradually ceased and the folded lava beds underwent slow metamorphic alteration. The principal feature of this alteration is the change of the augite into

hornblende—a change which is probably accelerated or facilitated by pressure. If it is true that this paramorphic change (using the term in a wide sense) is helped by pressure, it is probable that the accomplishment of this change very largely reduced the internal pressures in the mass. I may here refer to Mr. Bosworth Smith's suggestion that the metamorphic changes which these rocks have undergone may have given rise to the internal pressures causing strain and which he appears to use as an alternative or additional hypothesis to that of regional pressure, and I may observe that it merely presents the alternatives of considering either that pressure produces metamorphism or that metamorphism produces pressure, and in the present instance I prefer the former. Assuming then, that the change from augite to hornblende was produced or at least helped by the pressure under which the rock existed and that much of the intensity of this pressure was *pari passu* relieved by the molecular reconstruction of the minerals, the next point to be noted in the history of the rock is the intrusion of the great neighbouring masses of granite and gneiss. I have elsewere* shown reasons for supposing that much of the granite and gneiss on either side of this narrow belt of schists is intrusive towards the latter. If this view is correct the intrusions must have raised the schists to a considerable temperature probably with further modifications of the crystalline structure. On the cooling and shrinkage of the granite masses whatever regional pressure may have existed at the time of intrusion would be relieved and the heated schists would at the same time have cooled and contracted. Without

* *Vide.*—Appendix to Report of Chief Inspector of Mines—1899, and Records of the Mysore Geological Department, Vol. III, Pages 16 and 40.

going into the question of how many separate intrusions of granite or gneiss took place there appears to me to be ample opportunity in the above noted sequence of events for the strains due to the great original folding pressures to have been completely removed and even reversed giving rise to negative pressures in place of positive ones within the mass of the schists. Leaving out of account for a moment the formation of the fissures which the quartz reefs now occupy we may pass to the latter and final stage characterised by the introduction of the dolerite dykes. I see no reason for believing that these dykes cut their way up through a tightly compressed mass of schists or that the schists and adjacent gneisses were violently fissured along the two rectangular sets of planes now occupied by the dykes. The cleanness of the dyke walls, the comparative scarcity of fault breccias and the trifling amount of faulting which has taken place are all against such a view and I think it much more probable that the fissures are of the nature of a series of great joint planes produced by secular contraction, possibly assisted by gentle elevation from below, and filled quietly with molten dyke material. Again it is certain that at the time of intrusion of these dykes the surface level of the country was much above the present surface and that whatever tensional strains existed in the schists at that time have since been increased rather than diminished by the reduction of temperature consequent on the closer approach of the present schists towards the surface or more properly speaking the approach of the surface towards the schists.

Another indication of the tendency of the schists to contract is, I think, to be found in the behaviour of the walls and exposed surfaces underground. No matter how sound these may appear when first exposed, they rapidly

develop joints and scale off in large slabs and this effect is increased by the introduction of dry air. This appears to me to be more consistent with shrinkage of the exposed surface after removal of the resistance of the adjacent rock than with the tendency to swell and burst outwards under the influence of internal compression.

Finally as to the presence and influence of calcite, I agree with Mr. Bosworth Smith in rejecting this as an effective cause of the strains which produce air-blasts and in fact I am more than doubtful whether air-blasts are as marked when veins of calcite are present as when the schist is free from them and compact and homogeneous "Airing" of the rock where veins of calcite are numerous is common enough, but I doubt whether this phenomena can be properly classed with air-blasts such as occur in quartz, trap or solid black rock or if so only to a subsidiary extent.

Again the numerous veins of calcite appear to me rather as an indication of the tendency of the enclosing schists to contract and form fissures and the calcite itself, which is a soft mineral, is usually clear and crystalline and does not appear to be either capable of inducing great local pressures or of existing in such form in the cracks of a rock suffering from severe internal compression. All of these considerations lend colour to the suggestion that the schists have undergone contraction in the past and that they still may have local tendencies to contact further.

Quartz.

Lastly there is the question of the condition of strain in the quartz itself in which these air-blasts are observed more frequently than in either the schist or the trap. This

frequency may be an indication that the strains which produce air-blasts are more numerous or more strongly developed in the quartz than in the other rocks, but on the other hand such may by no means be the case and the frequency may be due partly to the fact that more work is done in the quartz and partly perhaps to the physical characters of quartz being more suitable for production of air-blasts than is the case with schist or trap.

Bearing these alternatives in mind let us consider the past history and present condition of the quartz. It is generally admitted that the quartz reefs are fissure veins occupying cracks or introduced along planes of weakness approximately parallel to planes of schistosity of the country rock. In places there are folds in the reefs, though their character has not been very clearly made out, and in places the quartz has a finely granular texture which may have been produced by crushing. Under the microscope the grains of quartz show strain phenomena, whatever their nature or origin may be, and it is generally admitted that the reefs have at some time or other been subject to considerable pressure. The important question, however, is whether such pressure still persists or whether in spite of a general relaxation of pressure portions of the quartz remain in a condition of compressive strain. I see no reason to think that the quartz is at present in a state of compression except such as may be due to the weight of overlying rock while the presence of joints and cracks in portions of the reefs, the introduction of calcite and quartz in secondary veins and the redistribution of metallic minerals along joint planes all seem to me to point to contraction after the formation of the reefs. It is true that there are *slikensides* in many places some of which may have been produced by pressures

before contraction set in and some of them may equally well be due to contraction itself causing differential settlement along weak or slippery planes. Above all, the appearance of the quartz in *Case No. 1*, described above and the formation of the cracks therein, appealed to me very strongly as being due to actual contractions going at the freshly exposed surface of the quartz rather than to the swelling out and flaking off of pieces by the expansion of an intensely compressed material.

Summary.

Two views of the origin of these air-blasts have now been presented, one of which is diametrically opposed to the other. It is admitted that the immediate cause of an air-blast is a condition of intense strain in a hard unyielding crystalline material such as a trap dyke, a crystalline hornblende schist or vein quartz. This condition of strain is not a constant feature in any of these rocks, but is locally developed under conditions of which we do not know the details Where these strains do occur they may be either positive or negative, that is, they may tend to cause the material either to expand or to contract when the surrounding envelope is sufficiently removed and one of the views above related adopts the former and the other the latter hypothesis.

The former hypothesis which is that adopted by Mr. Bosworth Smith is at first sight a very reasonable one owing to the evidence which exists of the rocks having been subjected to intense compressional stresses. In advocating the latter hypothesis, *viz.*, that the strains are tensional or tend to produce contraction, I have endeavoured to show that it is very possible that the great compressive stresses which deformed the rocks have long since been dissipated; that in one of the classes of material, *viz.*—that

of the trap dykes, there is no evidence of its ever having been severely compressed after solidification while it is probable that simple cooling and contraction has produced a condition of internal tension, and that in the other two classes of material it is quite possible that after the cessation of deforming pressures (that is, before the introduction of the trap dykes in Cuddapah times) secular cooling and contraction may well have led to the formation of joints and fissures and in the more solid portions to a tendency to further contraction which exhibits itself in air-blasts when portions of the surrounding envelope are removed. The latter hypothesis appears to me to be more in accord with what I know of the character of these air-blasts than the former, but even if this be admitted it can be regarded as only a very partial explanation of the phenomena.

We know practically nothing of the differences between air-blasts which occur in trap, schists or quartz, but I have no doubt that there are specific differences and further observations helping to define these would be very useful. It would be interesting to know whether trap dykes on other fields exhibit corresponding phenomena and whether the same have been noticed in quartz reefs lying in similar or in different enclosing rocks. Again, the fact that the schists have at one time been subjected to great regional pressure must not be lost sight of: even if such pressure is not the immediate or principal cause of the strain which gives rise to air-blasts it may have had an important influence on the character or distribution of the strains set up by subsequent contraction. I trust that in thus drawing attention to this matter at some length some valuable data for comparison will be forthcoming from other mining areas.

Quakes.

I pass now to a description of certain larger phenomena which are common in some of the Kolar Mines and which are usually reported as air-blasts. I think that they are distinct in character and origin from the air-blasts which I have already described and I purpose to distinguish them by the term "quakes" as in many respects they partake of the nature of small earthquakes. Speaking generally, a quake may be described as a local rupture of some portion of rock underground, giving rise to an earth tremor which may be felt on surface at a considerable distance from the point of origin—in some cases to between 3 and 4 miles. The quake is accompanied by a noise or report, frequently audible on surface for a mile or more from the origin, the precise character of which is rather difficult to ascertain. I have never heard one myself and the accounts given to me vary. Some people describe the noise as similar to the explosion of a heavy charge of gelatine; others as suggestive of the blowing up of a distant magazine, while others hear merely an indefinite rumble. Such differences are only to be expected and depend largely on the situation of the observer, whether within doors or in the open and whether near any shaft through which the sound may come or placed so that only the indefinite rumble which probably accompanies the earth tremor is heard.

These quakes appear to be of very frequent occurrence, but comparatively few are of such magnitude as to attract

general attention or to cause much visible damage underground. The larger and more striking ones are practically confined to two mines, *viz.*, Champion Reef and Ooregum, which occupy the middle section of the Champion Lode. The following record which has been maintained in the Champion Reef Office shows how many of these quakes have been noticed from November 1901 to June 1903.

Champion Reef Gold Mine.

REGISTER OF UNDERGROUND AIR-BLASTS. (*Quakes.*)

ate.	Hour.	Remarks.	Date.	Hour.	Remarks.
-11-01	5 a.m.	...	7- 6-02	10-40 p.m.	...
Do.	8 a.m.	...	25- 6-02	9-38 p.m.	Heavy.
-12-01	8-30 p.m.	...	25- 7-02	4-10 a.m.	Heavy followed by another slight.
-12-01	4 p.m.	...	...	...	...
-12-01	6-45 a.m.	...	28- 7-02	6-55 a.m.	...
Do.	9-15 a.m.	...	21- 8-02	11-45 a.m.	Do.
Do.	11-20 a.m.	...	2- 9 02	4 a.m.	Heavy.
Do.	11-27 a.m.	...	9- 9-02	8-35 a.m.	Do.
-12-01	4-35 p.m.	Heavy.	15- 9-02	1-45 p.m.	Do.
1-02	1-50 a.m.	...	Do.	4-42 p.m.	Do.
1-02	10-20 p.m.	...	25- 9-02	3-45 a.m.	Very heavy.
2-02	5-50 a.m.	...	6-10-02	12-10 p.m.	Do.
2-02	1-10 a.m.	...	18-10-02	12-40 p.m.	*Very* heavy.
2-02	8-10 a.m.	...	20-10-02	10-10 a.m.	*Very* heavy.
2-02	3-55 p.m.	...	24-10 02	11-52 a.m.	...

REGISTER OF UNDERGROUND AIR-BLASTS. (*Quakes*).—*Continued*

Date.	Hour.	Remarks.	Date.	Hour.	Remarks.
18- 2-02	11-30 p.m.	...	14-11-02	3-55 p.m.	Very heavy.
24- 2-02	1-39 p.m.	...	12-12-02	7-30 p.m.	Heavy.
7- 3-02	7-53 a.m.	Very heavy.	31-12-02	9-15 a.m.	Heavy.
25- 3-02	10-40 a.m.	Do.	23- 1-03	5-45 a.m.	Heavy.
7- 4-02	12-25 a.m.	Do.	29- 1-03	6-57 a.m.	Very heavy.
15- 4-02	8-45 a.m.	Do.	4- 2-03	8-10 a.m	Heavy.
26- 4-02	10-50 a.m.	Do.	22- 2-03	7-40 p.m.	Do.
30- 4-02	3-45 a.m.	...	8- 3-03	7-40 p.m.	Very heavy.
4- 5-02	12-45 a.m.	Very heavy.	11- 3 03	3-15 p.m	Do.
17- 5-02	5-45 a.m.	Two blasts in succession.	12- 3-03	7-15 p.m.	*Very* heavy.
18- 5-02	9-55 a.m.	Heavy.	14- 4-03	8-50 p.m.	*Very* heavy.
22- 5-02	10-28 a.m	...	7- 5-03	8-55 a.m	Do.
29- 5-02	8-23 a.m.	Heavy.	20- 5-03	10-12 a.m.	Do.
5- 6-02	2-35 a.m.	Do. and 3 blasts in succession	21- 5-03	2 56 p.m.	Very heavy.
...	...	...	2- 6-03	8-15 p.m.	*Very heavy.*
...	...	...	Do	8-16 p.m.	Slight.
7- 6-02	10-32 a.m.	Followed by another about a minute after	Do.	9-15 p.m.	Very heavy.
...	...	...	Do.	10-10 p.m.	...
...	...	...	25- 6-03	4- 6 p.m.	...
...	...	...	...	...	...

The great majority of the above were compratively small and were observed merely as slight tremors. Some

of the larger ones, besides being accompanied by a noise, have displaced roof tiles, shaken down plaster from the ceiling and caused furniture—such as a high book case—to rock.

Underground, the effects of the larger quakes may be illustrated by the following cases in which I have secured fairly reliable information and several of which I have myself inspected.

Case I.

My attention was first attracted to the subject by the report of a large air-blast which had occurred about the 760 feet level in Air shaft, near the southern boundary of the Ooregum Mine. The information received was that a very violent explosion had occurred which had blown out the candles of the men who were sinking the shaft a few feet below the 760 foot level; that large quantities of rock had been thrown about and that a considerable portion of the shaft had been damaged. The shock was both heard and felt at surface by several people.

Such severe effects appeared to me to be altogether beyond the range of an air-blast, so far as my knowledge of the latter went, and accordingly I visited the scene of the accident the day after its occurrence in company with the Superintendent, Mr. Bullen, and Mr. Arthur Taylor who was on the Field at the time.

The sketch (Plate II, Fig. 3) shows the situation at the time. Air shaft was being constructed on an underlie of about 55° through the old stopes near the southern boundary of the Ooregum property. Between the 660 foot and 760 foot levels the shaft was carried through a bar or arch of black rock which had been left standing in the old

stopes. At the time of the accident the bottom of the shaft was a few feet below the 760 foot level and the shaft had been completely timbered down to that level. On examination after the accident, it was found that about 30 feet of the shaft immediately above the 760 foot level had been bodily shifted southwards from 1 to 3 inches (Fig 2, Plate II), four of the end pieces (9 inch square new timber) on the north side of the shaft (A A) and several lining planks had been smashed like matchwood, the footwall of the shaft just below the level (C) was shattered and raised in great slabs and several props (B) in the 760 foot level were broken and displaced. Some 20 feet inside the level, a large gaping crack was found extending upwards into the arch of black rock immediately above some broken props. From the damage visible and from an examination of the plans, it seemed clear that the origin of the shock lay within the wedge shaped arch of rock occupying the corner north of the shaft and immediately above the 760 foot level. The pressure on this arch was no doubt very great owing to the amount of stoped ground to the north of it and above and below it and eventually it gave way causing the damage described. The shock felt at surface was probably due to the sudden give of the hanging wall and the sound may have been caused by the production of the large rent shown in the sketch. With regard to the fracturing of the footwall of the shaft at the point C, it is probable that an intense vibratory motion due to the momentary relief of pressure by the break up of the supporting arch of rock would be sufficient to displace several slabs of rock.

A study of this case led me to the conclusion that the quake shock was different from an ordinary air-blast, not only by reason of its magnitude, but in its mode of origin,

the former being due to the disruptive relief of strains produced by superincumbent presure, while the latter is, as I have already shown, due to strains existing in the rock material prior to and essentially independent of the development of pressure due to adjacent excavations.

In all the other cases of quakes which I have investigated I have found the same general conditions to prevail and each case has tended to confirm my original conclusion. I will now briefly describe a few other cases in the order in which they have occurred.

Case II.

On the 8th of November 1900, a big shock occurred in Garland's shaft, Champion Reef, between the 740 ft. and 840 ft. levels and was both felt and heard at surface. About 70 feet of the shaft was damaged, as shown by the dotted lines in Fig. 4, Plate II, the timber being shifted slightly to the north and most of the end pieces being smashed or bent. It will be seen that the shaft passes through a large pillar of quartz with much stoped out ground on either side and that the pillar is weakest where the damage occurred. The shock was no doubt due to the sudden giving way of the portion of the pillar between the 740 foot and 840 foot levels under the weight of the hanging. I did not see the place myself and cannot speak to the condition of the pillar after the shock; it was said to be much fractured at the sides of the shaft, but the outsides of the pillar were not visible as the adjacent stopes had been filled with dead rock. The fact that the stopes had been filled with deads is an important point. As soon as I had come to the conclusion that these quakes were due to the giving way of pillars under pressure, I pointed out the desirability of obtaining more support in the neighbourhood of passage ways by more extended filling of stopes

with black rock and much more filling has been done during the past two or three years than was the case previously, especially in Champion Reef and Ooregum. I hoped that careful filling would obviate these severe quakes, but this expectation has not been fulfilled. Quakes have continued to occur, though it is probable that their number has been reduced and probably also the severity of those which have occurred. Even if filling will not entirely obviate quakes, there can be little doubt that it helps to minimise their after effects and to lessen the chance of any section of a mine closing up as the result of the giving way of some supporting pillar.

Case III.

On the 3rd of January 1902, a fall of quartz occurred in the 940 foot level, Ribblesdale's shaft, Champion Reef, shown at x, Fig. 5, Plate II, the quartz which fell being indicated by vertical shading. This again was in a pillar of quartz through which the shaft passes, the stope immediately to the north of the pillar being filled with black rock. The displacement might from the situation be regarded simply as a heavy fall of ground, but from the statements of several Europeans who were in the level at the time as to the loudness of the report and from the fact that many of the timbers in the shaft alongside were broken and crushed, I am inclined to think that the fall originated in fracture due to crushing. I have known much bigger falls to occur without giving rise to any loud report or causing any noticeable shock.

Case IV.

A very severe quake occurred on the 28th January 1902 between the 1,060 foot and 1,160 foot levels at the southern boundary of Ooregum, See Fig. 3, Plate II. Here an old pillar (P)

forming the barrier between Ooregum and Champion Reef was being removed when suddenly a portion of the pillar (quartz) gave way and was badly shattered, much of the timber in the stopes on the north side was broken and displaced, a good deal of the hanging wall came away and the foot wall was shattered and thrown up in large slabs. There can be no doubt that the cause in this case was the sudden fracturing of a portion of the pillar under the top weight which is here very great owing to the extensive stopes on either side. The stopes were very heavily timbered, but no amount of timbering seems to form any safeguard in such cases, though it doubtless helps to keep the stopes from closing altogether.

Case V.

On the 30th January 1902 there was a big smash in Rowe's shaft, Champion Reef, between the 900 and 1,000 foot levels, see point x in Fig. 3, Plate II. The shaft here passes through a large dolerite dyke with some quartz left standing alongside. The quartz and some of the dyke gave way forcing about 50 feet of the shaft in a northerly direction (as shown by dotted lines), as much as 3 feet in places and smashing the shaft timbers. This is, I think, a clear case of pressure on the dyke and pillars of quartz owing to the excavation of the neighbouring stopes. The shock was distinctly felt at surface.

Case VI.

On the 11th March 1902 a very severe quake occurred near the 1,160 foot level on the southern boundary of Ooregum—see Fig. 2, Plate II. This was just below the place where case IV occurred and was undoubtedly due to the giving way of a further portion of the same pillar (P) which was being stoped away. Much damage was done to the timber and walls

of the 1,160 foot level and the shock was very strikingly felt at surface.

Case VII.

On the 5th June 1902 in the 1,040 foot level south of Glen shaft, Champion Reef,—see from x to y of Fig. 6, Plate II. This was a very severe shock and was felt in some of the bungalows at surface as far as two miles away from the shaft. It was feared that about 100 feet of the level had come together, but afterwards it was found to be only filled with debris. The roof of the level was heavily timbered and much of the stope above filled with deads. Much of the timber was smashed and the hanging and footwalls damaged. Near the point x several timbers were displaced and a quantity of deads ran down into the level. By far the greatest damage occurred below the pillar P where the timbers were completely smashed and the level quite choked up with rock and quartz. With the strong pillar P above, one would have expected the damage at this point to be less than elsewhere, unless, as I think must have been the case, the origin of the disturbance was due to the break up of the pillar itself. From the cross section through P (See Fig. 7) it is obvious that a large portion of the hanging is supported by the pillar with a certain amount of assistance from timber and dead rock and that the fracturing of such a pillar would produce the effects noted.

Case VIII.

A severe shock occurred at Champion Reef on the 13th of March, 1903 in a stope below the 900 foot level south of Rowe's shaft —see the spot marked y on Fig. 2, Plate II. This stope is on the south face of a large dolerite dyke which cuts obliquely across the reef from S. E. to N. W. and also underlies to the N. E. The 900 foot level goes through

the dyke. The stope at the time of the accident was about 33 feet deep below the 900 foot level and was about 20 feet in width and about 30 feet from N to S. (See diagrammatic sketch, Plate I, which gives a view of the stope from the footwall (eastern) side.) In the stope are two platforms resting on heavy stull pieces (18 to 20 inches in diameter); the 900 foot level runs along the near edge of the upper platform and passes through the dyke at about the point x (Plate I). The second platform is some 14 or 15 feet below and a ladder close to the footwall connects the two. The south end of the stope is occupied by a large *horse* of schist which divides the reef into two parts for some distance back. The reef comes together in the bottom of this stope and tails off in a wedge between the hanging wall and the oblique dyke, so that there is a large wedge shaped portion of the stope behind the portion of dyke shown in section on the right of the sketch. The lower corner of the dyke is represented as cut away to show the debris at the bottom of the stope.

A very severe shock occurred which was felt at surface for some three miles from the origin, the dyke was much fractured and much dyke and schist were thrown from the north end into the bottom of the stope burying two men. I visited the stope a couple of days after the accident in company with Capts. Poole and Treloar and a European timberman who was in charge of the stope and present at the time of the accident. The latter said that the noise and concussion was terrible, all lights were blown out and great quantities of rock fell, the whole place being filled with dust. He seemed satisfied that something in the end of the stope had gone off with a bang shattering the dyke and throwing pieces

of rock about. The two Captains were more or less of the same opinion and Capt. Poole drew my attention to two planks knocked out of the upper staging and sticking up obliquely as shown in the sketch and to a large piece of schist resting against the footwall of the level close by. He regarded this piece of schist as having been blown up from the stope below, through the staging, knocking up the two planks on its way. Before we left I think we agreed that such an explanation was unnecessary as well as being practically impossible. In the first place the piece of schist could have come, and no doubt did come, from the hanging wall above the level. It was too big (about 24 by 18 by 8 inches) to get through the hole in the staging without squeezing and neither the stulls nor the planks showed any signs of having been struck from below. Finally the planks were held in their oblique position by some large pieces of dyke which had fallen on their ends and there can be little doubt that it was the weight of these pieces of dyke which had originally raised them into that position.

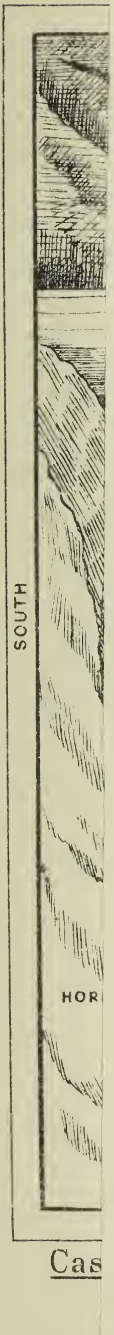
Again a portion of the lower staging had been knocked away during the smash and one of the large stulls (D) was found lying at the bottom of stope. I gathered from those who were with me that it was their impression that this damage was due to something bursting upwards and outwards from the lower end of the stope and throwing the timbers about. On the other hand, I myself am of opinion that the timber was knocked out by falling rock coming obliquely down from end of the stope behind the portion of dyke shown in the sketch. At the bottom of the stope some large blocks of rock (B) were found which consisted of a peculiar variety of schist which had evidently come from the wedge shaped end of the stope next

to the face of the dyke and which would have knocked down the stull when falling.

The most interesting point of the whole case is connected with a large piece of the same schist which was found resting against the timbers at the south end of the stope on the top of the lower staging (see A in sketch). This piece, which weighed over half a ton, was said to have been projected from the north end of the stope to the south end where it smashed the ladder shown there. As I had often been told of rocks being thrown about by these quakes, I examined this point minutely and came to the conclusion that there could be no doubt whatever that this large piece had been violently projected from the north to the south of the stope and that it must have travelled with a fairly low trajectory. I had the evidence of the timberman in charge of the stope that the piece was not there before the quake and that it was found immediately afterwards, in the position shown, with the ladder smashed and the broken off portion of the ladder crushed against the timbers behind it. Nothing had been touched when I saw it and the conclusion that the rock had smashed off the lower few feet of the ladder appeared to me to be indisputable, the northern side of the ladder was driven inwards so that the rungs projected on the outside of it for about three inches. Again, the piece of rock was identical in character with that in the northern corner close to the dyke where some loose and shattered pieces still remained. Also, it would have been quite impossible for such a piece of rock to have fallen into such a position from above, as the stope was closely timbered over by the upper staging with only a small ladder opening. I have no doubt whatever that the rock in question came from

the north end of the stope and that it was shot for a distance of thirty feet between the two stagings, finally breaking the ladder and being brought up against the timbers at the south end.

To sum up the whole case, I think that the evidence is clear, that nothing in the nature of an explosion occurred which fractured the rocks, but that a portion of the dyke yielded and was fractured under the steady superincumbent pressure of the overlying mass. It will be seen from Fig. 2, Plate II, that the dyke has been left standing as a bar or pillar in the midst of an extensive series of stopes having an underlie of 50 to 55 degrees and there can be no doubt about the great pressure thereon. The great cracks, some of them 1½ inches wide, running down into the dyke near its southern face confirm this view and other cracks were found within the mass of the dyke in the level. At the moment of collapse it would seem that a large quantity of dyke and of the schist adjacent to it on the hanging wall side had been thrown down into the stope and on to the upper staging and that the vibration produced by the production of a fissure by the bending and sudden yielding of portion of a dyke was so intense as to project a shale weighing about half a ton to the south end of the stope with a fairly low trajectory. Even if we allow that this piece did not travel clear all the way, but that it may have struck the lower staging and ricochetted along it, it will still be admitted that its original momentum must have been enormous. It is also worth while to note that notwithstanding the extent of fracturing which the dyke underwent, there appeared to have been little movement or deformation of hanging wall as a whole.



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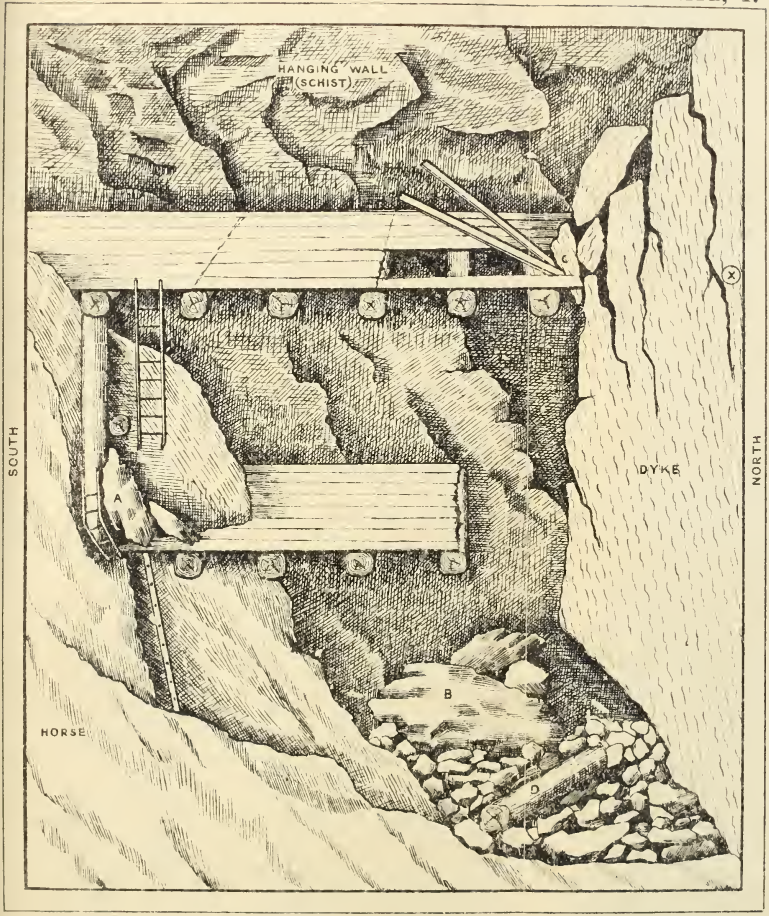


Case VIII. Stope below 900′ level, South of Rowe's Shaft.

Champion Reef

The next case occurred on the 20th March 1903 in a stope below the 1,085 foot level south of Rowe's shaft, Champion Reef,—See point z, Fig. 2, Plate II. In this stope the south face of the big dyke mentioned in the previous case was also exposed and suffered a considerable amount of fracturing, large flakes being thrown down from the face of the dyke into the stope. A well marked shock was felt at surface, but below ground no great amount of damage was done and I refer to the case chiefly because of its association both in time and place with the previous case. The same dyke was here affected in a similar manner though to a lesser degree and the latter case may perhaps be regarded as partly due to the former.

Case IX.

On the 2nd of June 1903 a severe shock was felt at Champion Reef at about 8 p.m., just about dinner time, and the shock was so noticeable as to cause several people to start up from the table and make for the doors. Plaster fell from the ceilings in several bungalows and the vibration was noticed as far away as Road Block—about 4 miles from the origin. The origin of the quake was in the 1,340 foot level, south of Ribblesdale's shaft, just where the cross-cut from Tennant's shaft meets the level. (See Figs. 8 and 9, Plate II). Just here there is a loop in the level, the eastern or main level being on the main lode which has been largely stoped out, as shown in Fig. 8, while the western branch or loop is simply a level in solid country there being no stoping above or below it. On inspection I found that the principal damage extended from x to y in the main level a distance of about 140 feet. All along here, the floor of the level was raised and the tram line bent and tilted up. The foot wall was much fractured

Case X.

and large slabs up to 12 inches thick were raised from it, the most marked effects being about the point C where there was only just enough space left between the displaced footwall and the hanging to crawl through. The hanging wall, as frequently noticed in other cases, was much less damaged than the foot wall, though many shales and large flakes had fallen from it. The roof of the level consisted of closely set stulls (14 to 18 inches in diameter) with lathing on top supporting the "deads" which filled the stopes above. Many of these stulls were crushed, bent or thrown down. The corner B of the wedge between the two levels had suffered considerably and the schist there was crushed and crumpled as though it had been forcibly compressed in the direction of the dip. In the loop level much damage was done at A on the foot wall side, where, as in the main level, the foot wall had been thrown out in huge slabs.

This case puzzled me considerably at the time of inspection, as I was informed that the reef had all been stoped out for considerable distance above and below the main level and I was therefore unable to see what had given rise to such a violent quake at this particular spot. On examining the plans afterwards with Mr. Stonor, the Surveyor of the Mine, I found that all the quartz had not been stoped out, but that a large pillar (P) had been left for support between the points x and y (see Plate II, Figs. 8 and 10). This at once enabled me to account for the quake in conformity with the conclusions which I had deduced from previous cases and I think there can be no doubt that this pillar had given way under the overload causing the damage which I have described. This case is therefore a further confirmation of the hypothesis that

quakes are due to the giving way of pillars under the superincumbent weight.

Conclusion.

The foregoing ten cases are, I think, sufficient to show that the larger air-blasts, or quakes as I prefer to call them, are essentially connected with and are in fact due to the sudden giving way of pillars or bars of solid rock in localities where much stoping has taken place. All of those of which I have received details have occurred either in Champion or Ooregum. I am informed that there have been some in the Mysore Mine, but they do not appear to have been severe and were not brought to my notice. None have occurred in Nundydroog or the other mines to the north thereof.

A complete explanation of the location of these quakes is not obvious and more information is required before it will be worth while to attempt an explanation. It is obvious however that all the cases which I have quoted above have occurred in areas in which much ground has been stoped out, the majority of them being situated not far from the boundary between Ooregum and Champion Reef. In Nundydroog the stoped out areas are less extensive and the reef dips more steeply than in Champion Reef and Ooregum and this may help to explain the absence of quakes in that mine, but the same explanation can hardly account for their absence or infrequency in the Mysore Mine where there are very extensive stopes and where the reef is flatter than in Champion Reef and Ooregum. The question of the general distribution of quakes therefore requires further examination.

Apart from this point, some explanation is required of the prevalence of quakes on the Kolar Gold Field as com-

pared with other Mining areas. The frequency as well as the severity of the phenomena described above are, so far as I know, peculiar features of the workings on the Kolar Gold Field which are not to be met with in other mining fields Such an impression may be partly due to ignorance of what occurs elsewhere, but I have not heard of or met with similar occurrences on numerous other fields with which I am acquainted. Possibly, however, the fact of my calling attention to the occurrence of these quakes here may elicit information as to their occurrence elsewhere and an examination of varying conditions under which they occur may help towards a better understanding and more complete explanation of them.

One of the first points upon which some more precise information is wanted is the character of the sound which accompanies a quake and the immediate cause of it. As might be expected, the descriptions which I have received very greatly, much of the variation being probably due to the location at the moment, as well as to the personal equation, of the auditor. There would no doubt be a considerable difference in the sound as heard by those below ground from that which is heard on surface, but there is often a doubt whether these two sounds have the same origin or vehicle of transmission. I have come across many cases in which a well marked report heard below ground has passed quite unnoticed on surface, which of course is not difficult to understand, and several more remarkable cases in which a sound distinctly audible at surface has failed to attract the attention of those working in the mine, and I am inclined to think that the sounds most frequently heard below ground are those due to the sudden rending or fracturing of rock which in most cases fail to make themselves heard on surface while the

sounds which are well marked on surface are due to vibrations set up by the sudden jerk of the hanging consequent on the fracturing of a pillar and transmitted to surface through the overlying rock with the production of an audible vibration as well as a perceptible tremor.

The next point for enquiry is the reason for the prevalence of these quakes on the Kolar Gold Field when compared with other fields. In the Champion Reef and Ooregum Mines very large sections of the reef have been removed down to a depth of about 1,700 feet, the workings being on an underlie of about 50°, and undoubtedly the extent of these workings and the comparatively low underlie tend to produce great pressure on pillars and other supports of the hanging wall, more so than perhaps than is the case in the majority of gold fields. On the other hand there are certainly both gold and coal fields where the pressure on pillars must be as great as or greater than here and where, so far as I know, quakes are not prevalent and the inference must be that though pressure is essential, it is not sufficient to cause quakes except under special local conditions. Such local conditions are, I think, to be sought in the physical characters of the rocks forming not only the pillars but also the adjacent country rock and it will probably be found essential that these rocks should be both hard and brittle within certain limits, that, in fact, they should be capable of withstanding very considerable pressure without appreciable deformation, but that once a certain limit of pressure is reached, they should yield suddenly. The conditions are remarkably fulfilled on the Kolar Gold Field by the quartz, the dykes and the black rock, that is to say, by all the rocks present. If the material forming the pillars is not sufficiently strong or if the hanging and

foot walls are sufficiently pliant or plastic, the latter will tend to close together or to close on to filling materials as the stopes are extended and thus save the pillar from much of the superincumbent weight, and in both of these cases the tendency would be to obviate quakes. If on the other hand the hanging wall were practically rigid, all the superincumbent pressure would be borne by the pillars and none by the timber and filling in the adjacent stopes. With a highly rigid hanging wall the tendency will still be to leave much of the pressure on the pillars with some on the timbers and little, if any, on the filling material in the stopes and this is probably the condition in those places in which the quakes occur on the Kolar Gold Field. Here the pillars are undoubtedly of strong and rigid materials whether they are of quartz or dyke or black rock. The black rock which forms the hanging is also in many places a strong and rigid material which does not yield readily as a whole. Not being perfectly rigid, it yields to a certain extent as shown by the crushing of timbers, and in weak places it may come down firmly on to a filling of dead rock, but in other places the hanging is sufficiently rigid for a large area thereof to be practically supported on a pillar, with the result that when the limit of crushing weight for the pillar is reached the latter yields suddenly causing a quake. I think that some such conditions as these are necessary for the production of the quakes which I have described and will suffice to account for them. The local conditions even in any one mine must of course vary very greatly and it by no means follows that wherever there is a pillar there will be a quake. It has been noticed in several cases that the fact of some the stopes being filled with deads has not prevented the occurrence of quakes, but this is no

reason why such filling should be discontinued or abated. It is probable that the filling has in many cases greatly diminished the after effects of a quake and that by taking up the pressure over a larger area it will tend to diminish other quakes in the same locality. If it is true that pillars are an essential factor in the production of quakes the obvious policy would be to leave no pillars and to support the hanging walls of stopes by timber and deads only, allowing the worked out parts to close down as soon as possible. But there are many practical difficulties in the way of carrying out such a policy. In some cases what are virtually pillars are left, because the stuff forming them is not wanted and their removal means expense—for example transverse dykes and patches of barren ground—but on the other hand I would recommend mine managers to consider the advisability of not leaving pillars for the express purpose of supporting ground, but rather as far as possible to do without them and to trust to keeping open any necessary spaces by means of timber and careful filling in of dead rock.

There is one more point to which I may refer. It has frequently been noticed in the case of large quakes that the foot wall has shown much more signs of damage than the hanging wall and the way in which the footwall has been thrown up in places has, I have no doubt, much to do with the popular impression that the quake was due to something in the footwall which caused the rock to burst up. The bursting up of the footwall is, as I have tried to show, an after effect of the quake, a side issue in fact, and not the primary feature. If we suppose a pillar to be under a very heavy compression we may regard the hanging and foot walls as slightly dented at top and bottom thereof. When the pillar yields there will be a momentary

relief of pressure during which the dented portions of hanging and foot walls will spring inwards setting up elastic vibrations. The vibration in the hanging side is doubtless what is left and heard at surface and it tends to shake off loose pieces from the hanging wall in the vicinity of the quake. As the hanging wall is usually kept fairly clean, the loose being taken down frequently, there is not very much loose to be shaken down. With the foot wall it is different. Here the rock gradually fractures and loosens in slabs which are not removed with the result that the vibration consequent on the yielding of the pillars throws the slabs outwards from the wall giving the impression of great local disturbance. The disturbed condition of the foot wall may often give the impression that the shock has originated there although it may really be due to an adjacent pillar hidden behind the timber. In the few cases in which the pillar itself has been exposed to view, the damage thereto has been striking and obvious.

AIR BLASTS AND QUAKES. CHAMPION REEF AND OOREGUM MINES.

Plate II.

Fig. 1

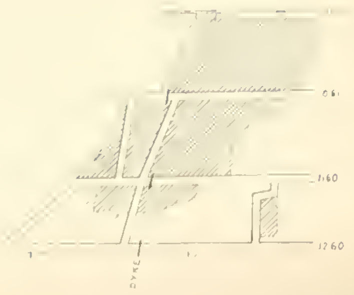
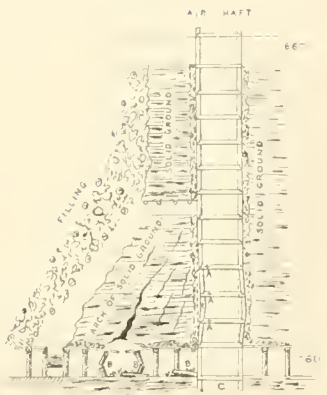


Fig. 2



Scale 40'=1"

Fig. 4

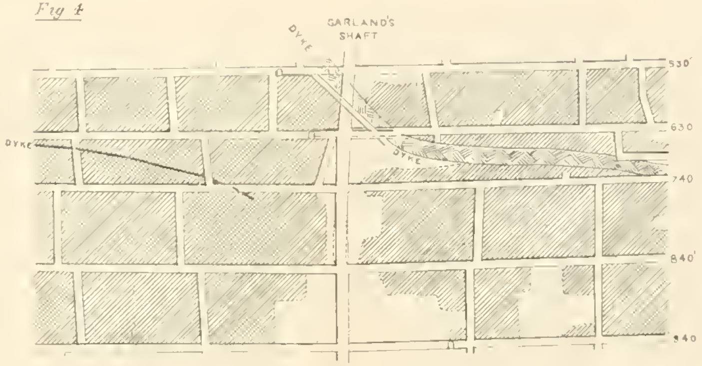


Fig. 8

Fig. 3

Ooregum

Champion Reef

Fig. 5

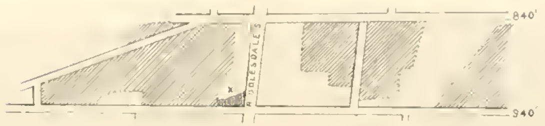


Fig. 6

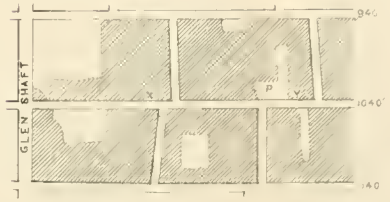


Fig. 7

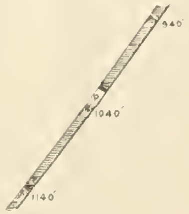


Fig. 9

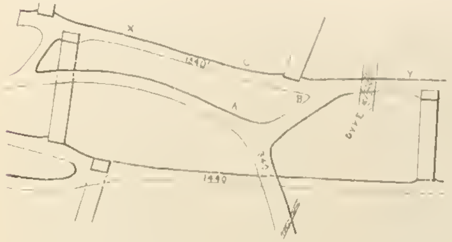
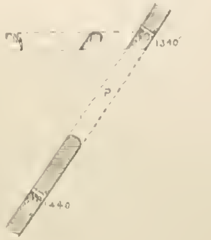


Fig. 10



Scale 60'=1"

REFERENCE

Slopes

General Scale 120'=1"